# Possible Sterilization of City Water.

By R. Meade Bache.

(*Read before the American Philosophical Society, April 17, 1891.*)

It is an open question whether the characteristic acidity of the digestive fluids is or is not efficacious in destroying pathogenic germs entering the stomach. But it ought to be evident on both sides that neither extreme can represent the truth, even if the digestive fluids possess that general property. It is certainly, on one side, too much to assume that, not even in a perfectly healthy stomach, are those fluids sometimes capable of eliminating such germs from the system, and, on the other, that they are always, in sickness or in health, capable of performing that task. So little vitalized are micro-organisms in their resting-stages, that it is easily conceivable that, when masked by food and water, and when the human system is in a weak condition, many escape the possibly destructive action of the healthiest digestive secretions.

It would, additionally, be an unwarrantable assumption, even if the healthy stomach were proved to be able always to neutralize the morbific action of pathogenic germs, that they find their inevitable path and exit, with or without vitality impaired or destroyed, dead or alive, through the alimentary canal; for in point of fact we know that one kind, at least partially, takes its disastrous course directly into the lungs. When the infinitesimal size of micro-organisms is considered, and when also is considered how varied is the character of the parts with which they must come into contact upon passing the œsophagus, it will readily be perceived that, even if they escape the sometimes assumed destructiveness of the digestive fluids, they must often be absorbed into the blood by other tissues as well as by those of the lungs.

If so believing, we should perceive at the same time that it is hopeless to contend, except by palliative sanitary measures, against the invasion of pathogenic germs through inhalation; but that, on the other hand, especially as our food cannot be sterilized wholesale, we should deeply consider the possibility of contending with them by means of the wholesale sterilization of water, which enters alone, or as the largest constituent, into our drink. That this has heretofore not been attempted is all the more remarkable, because it is believed by many persons that some of the worst forms of pathogenic germs reach us through the medium of drinking water. Inasmuch, therefore, as success in sterilizing the drinking water of a large city might be of untold benefit to it, it would be well that certain experiments were tried to that intent, upon the assumption that, be the pathogenic germs in a particular water many or few, they become, when received into the alimentary canal, whether or not large numbers of them are successfully dealt with by the stomach, injurious to the human economy.

Inherent in the Anderson iron process for the purification of water is a danger which, therefore, cannot be eliminated. In all processes there is a danger line which human foresight seeks to avoid by a safety-margin, which, in the long run, and in the nature of things, is a substantial guarantee against harm. But there are processes such, from their character, combined with the chapter of exigencies and the chapter of accidents, that they have but a small margin of safety. I place the Anderson process in this category, as an experience at Berlin, showing the danger that may result from the overworking and freezing, or both, of open filter beds, even if so acted upon and cleansed as they are intended to be by the Anderson process, fully warrants me in doing. Moreover, it should be incidentally mentioned that the process is not applicable to the constitution of all waters, or adapted to climates that have always, or are liable to, severe winter cold. It is said, however, upon excellent authority, based upon the indisputable evidence of microscopic examination, that by the process micro-organisms have, under the limiting conditions hereby implied, been neutralized in the proportion of 50,000 to about 20, virtually in the proportion of 50,000 to 0. But, coincidently with this result, which must obtain under favoring circumstances, there also always exists danger in the process through carelessness and neglect in filter cleansing, and necessity without law of overworking the capacity of a filter. As a finality in the process the ferric hydrate generated, blended with organic matter, is precipitated in a flaky, coagulated condition to the bottom of the water, the sand filter-bed of the settling reservoir, where, resting chiefly on the surface, the filter is therefore more readily than usual cleansed. The process therefore makes no pretense to destroy the micro-organisms, but merely to neutralize as much as possible their injurious action in the human economy, simply by entrapping them. What I contend, however, is that the best process of sterilization is that which does not seek to entrap micro-organisms, with the inseparable danger of their partial or almost entire escape alive, but that which, with abstention from their purposive arrest, kills, and allows them as free passage as possible to the stomachs of city dwellers. It will probably be thought at this point, with a very usual misconception, as that which we have in the Anderson process has proved quite efficacious, whereas that of which I speak is but an ideal, perhaps impossible of attainment, that I am proposing to accept a shadow for the substance of a thing. I would grant the cogency of the thought, had I ever intended to make denial of the excellence of the Anderson process, and proposed to offer a possibility in exchange for a reality. But, having taken neither of these positions, I do but state the case in the abstract, and the truth of it in that form being admitted (and I do not see how it can be denied), I have but to add before proceeding that, excellent as is the Anderson process, within its acknowledged lines, it would still be well to consider if the ideal one is not capable of accomplishment by the means which I am about to suggest.

About two years ago it occurred to me that before experimenting with bacteria, with reference to killing bacilli established in the human body, and with reference to the sterilization of city drinking water by electricity, I would pass a current through some water containing protozoa, and observe how much is required to kill them. With this purpose in view I took a glass tube of four inches in length and five thirty-seconds of an inch in calibre, and partially filled it with water teeming with protozoa from hay-infusion, which had previously been examined by me under the high power of a one-tenth microscopical objective, commanding a large field with an immersion lens, and depending upon which of two eyepieces was used, magnifying from five hundred and fifty to six hundred and fifty diameters. When both ends of the tube had been plugged up with brass eye-screws wrapped with paper, leaving their ends exposed in the tube, the volume of infusion intervening between the ends of the poles thus formed was only two-thirds of a cubic centimeter, and the distance between the poles only three inches. The electro-motive force at my disposal in my galvanic battery—only about thirty volts—was too small, and the resistance too high under these conditions for me long to hope to affect the protozoa in the tube by means of the current. The smallness of the volume of fluid in which the electricity could find play, and the liberation of hydrogen which could not escape or recombine, were together the cause of this ; the resistance from the latter cause proceeding by great leaps when a higher current was eventually employed. With the infusion the resistance was far less than with pure water, but still far too great to allow of much current, owing in sum to the small volume of liquid and to the increased liberation of gas in it as compared with that liberated in water. The current was so slight that at this point of time I was satisfied that if I were not able thus to destroy the vitality of the protozoa—and that was proved by microscopical examination—*a fortiori* it was not to be imagined that the vitality of schizomycetes in water could be arrested, because I had assumed that they would be more difficult than the other organisms to destroy, a conclusion which I do not now think warranted by my final investigation upon the basis of experiment. I therefore desisted from experimenting, and did not resume it until the work of Dr. Griffiths on micro-organisms came under my eye, from which I learned that he had killed bacteria with a very small current in media of a fluid character. I then resumed my experiments upon the basis of my previously enlarged experience, that a considerable volume of water is needed for the play of electricity, and that even a slowly increasing bubble of hydrogen in a closed tube, although far from effecting embolism, nevertheless produces rapidly cumulative resistance. Every one who deals with batteries or who is well-read in electricity knows in a general way of these phenomena ; I am merely referring to the exaggerated degree in which they manifest themselves under the specified conditions. I was well aware that for a given ampèrage, a given electro-motive force, a given character of liquid, a given temperature, and a given distance between poles, the resistance to a line of force of electricity is an absolutely fixed quantity. But as my final object, as will eventually be seen, was to charge a large volume of water so that upon being charged the electricity would concentrate with intense energy towards the opposite pole, it became necessary, even in laboratory experiments, to avoid action where the phenomena appear in an exaggerated adverse form. I therefore next proceeded to deal with small but unconfined volumes of liquid.

With the Wheatstone Bridge, with an electro-motive force of one hundred and ten volts, and one ampère of current, I found the resistance at two inches between the poles, placed vertically in a hay-infusion, in a round glass dish about five inches in diameter, to be 1560 ohms. Making the liquid a little shallower, the other conditions remaining the same, the resistance rose to 2120 ohms. In a very narrow, rectangular receptacle, the other conditions remaining virtually the same, the resistance rose to 3000 ohms. The poles being then placed in water, not in the infusion, in the round glass dish, the other conditions being the same as those in the first experiment, the resistance became 18,400. Slightly increasing the depth of water in the dish, the resistance sank to 13,000 ohms. These rude experiments were followed by a series conducted with two beautifully finished wooden, shellaced boxes, of exactly the same length and depth in the clear, but one of them of only half the width in the clear of the other. Thus was obtained with precision in the larger of the two (but, of course, the same consequence would have ensued with the smaller), by alternately making it exactly one-half full, and then full to the brim, the result that the volume thereby vertically obtained reduces by one-half the resistance of the lesser volume. Thus, also, by filling both boxes to the brim was obtained with precision the result that double the volume of liquid horizontally obtained reduces by one-half the resistance of the lesser volume. Therefore it was demonstrated that resistance in water, as well as in metal, is inversely proportional to volume as determining cross-sectional area, whether increased by vertical or horizontal extension; that is, is inversely proportional to cross-section, as dependent upon volume; and that in whichever of these two directions volume is gained, it introduces, proportionally, freedom of propagation of the electric force in and about the imaginary right-line joining the poles.

The result of a series of experiments, with the poles placed apart at 2, 4, 6, 8, up to 12 inches, showed that the resistance, whatever it may be, varies *directly* as the distance between the poles, a result identical with that in electrically charged wire, illustrating a law which should have been expected to hold good whatever figure and volume the lines of force between the poles might assume and occupy. The experiments clearly proved, too, that the resistance of water is very much greater than that of an infusion not seemingly dense.

There seems to be with some persons a belief that water is a good conductor, because current electricity so readily discharges itself by means of moistened surfaces. But current electricity so discharges itself through a film of water covering non-conducting surfaces in default of any other conductor whatever; and static electricity, for the same reason, readily vanishes through aqueous vapor, because of the fact that the vapor impairs the resistance of dry air as a dielectric. Yet electricity, in these two manifestations, acts thus, of course, not from choice but from necessity, taking, however imperfect, a path of conduction when there is no other, and the better of two paths when they differ, in proportion to their relative conductivity. Other persons imagine that water is a worse conductor than it really is. Any one who uses a hydro-rheostat well knows the highly resistant property of water to the electric current; but as free and in large volume it is not practically so resistant as it is sometimes thought to be, as any one may prove for himself by the rude experiment of plunging in an ample basin of water the sponge of one reophore of a medical galvanic battery, yielding from thirty to forty volts, while the sponge of the other reophore is placed on the back of the hand submerged in the water at the distance of four or five inches. The hand, the most callous part of the body except the heel, feels the current distinctly in every part, and if it has but the smallest abrasions of the skin in places remote from each other, the electric current makes them sting, finally condensing strongly at the pole on the hand.

After trying the experiments described, I flashed one hundred and ten volts through a glass tube, with half of a cubic centimeter of hay-infusion containing protozoa, with the poles half an inch apart; and also flashed one hundred and ten volts through a looped wire going from top to bottom of a small bottle containing four centimeters of the infusion. In neither case could subsequent microscopical examination detect that the organisms had been affected in the least. The whole of the current, of course, passed through the organisms in the tube. In the case of those treated with the looped wire it was only the residual force, which the wire did not carry, that they encountered. That under these conditions the wire does not carry all the electricity is shown in the forthcoming description of experiments, in which the work of killing bacteria was successfully accomplished with looped wire passing through fluid media, and carrying only a very small force, but for a considerable time. With so much electro-motive force as I used—one hundred and ten volts—I could not allow the discharge through the micro-organisms to be more than momentary, else they would have been destroyed for certain by the concentrated products of electrolysis.

Two main conclusions seemed to me from the beginning of my experiments to be justifiable. The first of these was that, inasmuch as protozoa have no nervous system, and do not seem to be injuriously affectible by the electric current (barring its action under conditions such as generate heat almost exclusively), we are accustomed to think erroneously of the current as capable of affecting and endangering all sensation and life, solely because of our own possession, and knowledge of the possession among other animals, of a nervous organization upon which stress may be put by the current. It seemed to me that the last experiment proves what is currently believed, that an animal protoplasmic organism has, *ipso facto* of its being protoplasmic, no nervous system. The second conclusion at which I arrived was that, if protozoa of the kind with which I had dealt are not easily killed by the electric current, it would be hopeless to think of destroying schizomycetes, except by a force which, for the practical purposes that I had in view, it is impossible to apply to them, especially as, in the pleomorphic forms assumed by some of them, it is notorious that they possess latent vitality difficult to extirpate.

I am still inclined to hold to the first conclusion, as justifiable from my experiments as far as they have even now gone, that animal micro-organisms, submerged in water or any other liquid, are not susceptible to injury from electric current approaching in force the highest that I used (which may be regarded as prodigious when the minuteness of the organisms attacked by it is taken into consideration), and that perhaps they are not susceptible to injury under those conditions from any current, however high. But, as to my first conclusion, I have since found myself, upon reading the work of Dr. Griffiths, egregiously in error through the false inference that I had drawn that, because the electric current did not destroy protozoa of the kind with which I was dealing, therefore bacteria would not be destroyed by it, at least within the bounds compatible with human life or well-being. It seems, however, that vegetable protoplasm, at least of the fungus kind, acts differently from animal protoplasm under the influence of the electric current. After reading the results of Dr. Griffiths, I gladly reverted to the intention with which I had set out in my experiments, of being able to suggest means by which bacilli forming a nidus in the human body could be destroyed and water supplied to cities could be sterilized for drinking purposes.

The author to whom I have referred is Dr. A. B. Griffiths, Fellow of the Royal Society of Edinburgh. He remarks that the full details of his experiments with electricity on bacteria are to be found in Volume xv of the Proceedings of the Society. In making the experiments he seems to have had no ulterior object in view but the gaining of information as to what amount of current would destroy certain micro-organisms. The wood-cut which he gives at page 177 of his work, *Micro-organisms*, represents a faradaic, not a galvanic battery, as the generator of the electromotive force used in his experiments. At the beginning of mine I used both the galvanic and the faradaic battery. The receptacles in which Dr. Griffiths placed pure cultures of different bacteria were simple, broad-based, short bottles, in which were fitted from top to bottom of each bottle a single loop of wire in free electric liquid communication with the micro-organisms. He does not in any case give the resistance in ohms of the media employed in the cultures.

The bacillus tuberculosis was killed by 2.16 volts, the bacterium lactis by 2.26 volts, and the bacterium aceti by 3.24 volts. The electric current was allowed to pass for ten minutes, and the temperature of the laboratory during the experiments was 16 C. (60.8 Fah.). In another series of experiments, bacillus tuberculosis was killed by 2.16 volts, bacillus subtilis by 2.72 volts, and bacterium allii by 3.3 volts. The current, as before, was allowed to pass for ten minutes, and the temperature of the laboratory was 17 C. (62.6 Fah.). In the first series of experiments no growths appeared from inoculation in fresh nutritive media, after an incubation of twenty-five days, with the thermometer at

38 C. (100.4 Fah.); and in the second series, similarly treated, no growths appeared after an incubation of twenty days, with the thermometer at 35 C. (95 Fah.). As before incidentally mentioned, all of these experiments were made with wire looped in glass bottles. Consequently all the electricity that attacked the microbes away from the wires was the residuum which the wires did not conduct, necessarily by far the lesser portion; and as the minimum of force was not sought or obtained, what is needed may be a mere fraction of the time and force actually employed. With so small a current as that used, and with the considerable volume of the respective liquids employed—which latter point the wood-cut shows—detriment to the organisms from products of electrolysis may be deemed inappreciable.

It has therefore been demonstrated that certain schizomycetes can be killed in a short time by a low current. Presumably all others can be killed in an equally short time by an equally low current; which was the assumption with which I had set out at the beginning of my own experiments, looking primarily to destroying pathogenic germs in the human body, and secondarily, to rendering them innocuous through the sterilization of water for drinking purposes. I therefore ask myself why, if a very low current, passing for a few minutes, can destroy bacteria in a bottle, should not a much higher one, administered repeatedly for the same time, be sure to destroy them in the human body? Daily, in the course of electro-therapeutic treatment, ten, twenty, twenty-five, and many more volts are administered to patients, avoiding only strong or continuous application of the current to the pneumogastric nerve, on account of the inhibitory action of the heart thereby provoked. But I will not pause just at this moment to speak more fully to this point, but will here confine myself to the main subject of this paper, clearly set forth by its title and the tenor of the preceding remarks. Reverting to the question of the sterilization of water for the use of cities, and with the new light upon the subject, which, as it appears, I might have gained for myself, but for having been diverted from my course by a false inference, I am constrained to ask my hearers, as I have asked myself in this case also, why the attempt should not be made to destroy bacteria wholesale in the drinking water of large cities by the method previously foreshadowed.

The means at our command seem to me ample. It is true that we cannot electrolize successfully a large reservoir of water, for in that the electricity would be too diffused to be effective. It is true that, in pipes from which water is flowing into or out of the reservoir, its germs would not be subjected to attack for more than a second. It is true that the resistance that we should have to overcome in water would be large. But, on the other hand, it is also true that the electric current that we have at our command is capable of indefinite increase. The electro-motive force of a few thousand volts (there are dynamos that generate ten thousand) thrown athwart a pipe of proper dimensions, would probably paralyze every bacterium in its path, more than compensating by force for slight duration in time as compared with the ten minutes adopted in the experiments of Dr. Griffiths, as to which it is imperative to remember that they did not determine either the amount of current, or of time required, for the destruction of the bacteria experimented upon; and, consequently, it will be observed, both force and time needed are probably very much less than his experiments on their face apparently demonstrate.

If lines of water-delivery as well as those of water-supply were subjected to the attack of the electric current, the severity of it would be more than doubled for the organisms. It would be immeasurably increased in severity; for experiments at the very beginning of bacteriological investigation clearly showed that the best mode of destroying bacteria involves the principle of repeating relatively moderate attacks upon them at intervals such as find them partially recuperated, and assail them in this the period of their least resistant vitality. The method to which I allude is that of repeated boiling of slight duration at moderate intervals of time. That they can bear this apparently severe process at all shows the protective influence for them of any fluid immersion within the chemical character that does not wholly ignore the difference of habits among their different species, and water seems to be a medium inclusive of them all. The principle involved in the mode of attack mentioned is the same as that involved in the mode of destroying bacteria here suggested. Taking it in connection with the facts that a reservoir represents a large volume of water, only a part or a few parts of which are being momently drawn upon for supply, and that many germs are constantly passing through natural phases of relatively less vitality, infinitely below that in which they, if pathogenic, being received into a favoring host, so vigorously form ptomaïnes, to their self-destruction as well as that of the host, it would seem that, if upon issuing from as well as upon entering a reservoir, the water were attacked in pipes from poles all but encircling them, with an electro-motive force of a few thousand volts, all germs must reach the denizens of cities supplied from such a source, wholly innocuous, because they would be dead.

It need hardly be said that, if the poles were placed opposite to each other on a heavy metal pipe conveying water, the electricity, seeking lines of least resistance, would not pass through the water at all, but around it, through the great mass of the pipe. But it should be obvious that it is easy to adapt to the place of electrical attack of a pipe a simple contrivance consisting of a section of the same diameter as that of the pipe, insulating the poles from each other, and both from the general line of the pipe. A plan that might at the first blush appear to some persons better, as not entailing thus radically breaking the continuity of the main pipe, would be to have two series of metallic insulated screws, representing by position two opposing arcs, the individual screws of which should enter and pass through corresponding holes in the pipe, the ends of the screws being uninsulated. But this plan would not do at all. The experiments described have proved the resistance of water to be so great that a large volume of it is required for electricity to pass easily through it. Consequently, in overcoming the resistance of water in a metal pipe with poles attached, in the form of insulated perforating screws, part of the electricity would, in making large excursions, be received and conducted to the poles by the metal of the pipe, instead of reaching them entirely through the water. But, if the pipe were interrupted by a non-conducting section, of length to be determined by the diameter of the pipe and the electro-motive force to be used, then those excursive lines of force would eventually fall into the determinate direction of the poles entirely through the water. We see this action clearly illustrated in the previous experiment, where, in open vessels, resistance to the current rapidly diminishes as we increase the volume of the liquid. We see the same thing also clearly illustrated in the case of the hand submerged in the ample basin of water, where the remotest abrasions of the skin sting from the current, finally emerging with condensed force at the pole resting on the submerged hand. In a pipe with a properly calculated non-conducting section, the lines of force would play freely inside of the pipe, occupying and limiting there a rounding imaginary space, varying in figure with every change of force, but always, of course, having its apices at the poles, approaching which, and especially at which, would be concentrated their intensest energy.

If the full significance and legitimate outcome in conclusion from the experiments that have been detailed have been perceived, it will have been realized that, although water acts like wire with reference to conductivity, through length, cross-sectional area, and temperature—exemplifying the law of conduction by and resistance to the electric current, with reference to volume, however disposed—the difference between wire and water, notwithstanding that metal has great conductivity and water very little, is enormous with reference to difference of capacity. We have but to determine, first of all, what electromotive force is needed for the purpose of destroying germs in water, assuming that they are thus destructible, and then, upon that basis, determine what the length and cross-section of non-conducting pipe should be to accumulate and discharge the force required. One could charge a constant stream of water in an insulated pipe as never wire nor any congeries of wires nor any metallic deposit on earth could be charged with electricity; for whereas all these would soon reach their utmost capacity for localized energy, an insulated flowing pipe has back of it all earth ready to receive and effectively return the force transmitted. We, however, need for our purpose at most only a small area of that vast space. But yet it is true, and a striking exemplification of the stated fact that, given a dynamo of far less than infinite power, with poles astride an estuary's living stream, so wide, so deep, that the earth there would not fuse before a fiery blast engendered by resistance, and connected as those waters are with every drop in every brook, the encircling oceans, and the interlying land, it would send its impulse thence over the whole uninsulated globe, and backward, in myriad lines of force, with all but synchronous and omnipresent thrill.

I stated at the beginning of my discourse that it is an open question whether or not the stomach is capable of destroying pathogenic germs. In that, of course, is involved the other open question, whether or not ordinary drinking-water is the source of disease. I have properly spoken of the questions as open ones, because so many persons are enlisted on opposite sides that I cannot venture without arrogance to decide them authoritatively. The tenor of the preceding remarks, however, must indicate that, personally, I believe drinking-water supply to be ordinarily one of the largest factors in the causation of some zymotic diseases; but lest I may have left it in doubt that I hold that view, I here state it explicitly. I have, I confidently believe, pointed out one way in which the evil may be abated, and perhaps neutralized; and this without disparagement of the efficiency of subsidence basins in their adverse influence upon bacterial dissemination. As to this (with the exception of treatment with iron) the last remaining factor in the production of pure drinking-water, I shall be glad to take a more opportune time than the present occasion, when I have so long engaged the attention of the Society, to prove directly, from my still later experiments and observations, what seems directly proved by the statistics of prevalence of typhoid fever in Philadelphia and elsewhere with reference to areas of different water-supply, that subsidence basins are also an important factor in the health of a city, not only relieving water of impurities in it, represented by alluvial and effete matter in suspension, but also relieving it in a measure of the impurity due to simultaneous deposition of the bacterial bearers of poison to our homes.

As to our ability to destroy the bacillus tuberculosis in the human body, by means of percutaneous administration of the electric current, I hope that I may be allowed to say a final word. I cannot see, as I have already remarked, why, if it can be killed in a bottle with a mere fraction of two volts (as I have shown by the experiments of Dr. Griffiths that it must have been killed), it cannot be killed in the patient suffering from tuberculosis, by the enormously greater electro-motive force that the body is capable of receiving without detriment in a concentrated form. This statement, however, is not intended to imply that the current would be capable of curing a case of tuberculosis which had involved caseous degeneration of the parts. If it did, it would also imply that to my mind electricity is creative. Electricity, however, although not creative, includes among its manifold and marvelous properties not only dynamic power, but attributes regenerative of vitality, and with these two it is capable, if the experiments of Dr. Griffiths are to be relied upon, of killing the bacillus tuberculosis in the living human body, in case the lesions of the disease have not seriously impaired electric conductivity in the parts morbidly invaded; and capable also of contributing to restore healthy function to them, and thence normal structure. It remains for physicians to make the essay here indicated at no expense or risk whatever. If the treatment prove to have any virtue in it, it would apply to other bacterial diseases besides tuberculosis.

In regard to the essay with reference to the sterilization of drinking-water, experiments could be made at no great labor and expense compared with the vast interests at stake in a large city. Through microscopic tests would soon be set at rest the question as to whether to any, and if to any, to what extent germs could, by the means described, be destroyed in city water, and scrutiny of the health of the city, within the lines especially of certain diseases, through comparison of present with past records, would in successive years have its own independent and conclusive tale to tell. I pledge Philadelphia prospectively in a bumper of pure water more worthy of celebration than the best Falernian wine.

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*Obituary Notice of P. W. Sheafer. By J. P. Lesley.*

(*Read before the American Philosophical Society, April 3, 1891.*)

Peter Wenrich Sheafer was born at Wiconisco, in Dauphin county, Pa., March 31, 1819. His father, Henry Sheafer, was afterwards President of the Lykens Valley Railroad Company, and Superintendent of the Lykens Valley Coal Company, mining the finest quality of anthracite coal, at the west end of the Southern Anthracite Coal field. The discovery of the Lykens valley coal bed in the body of the Pottsville Conglomerate was one of the astonishing incidents of Pennsylvania geology, and enabled the Sheafers, father and son, to establish a great trade in anthracite coal upon the line of the Susquehanna river as far as Baltimore.

Peter Sheafer was engaged at various times in his long professional life in following the outcrop of this interconglomerate coal around the edges